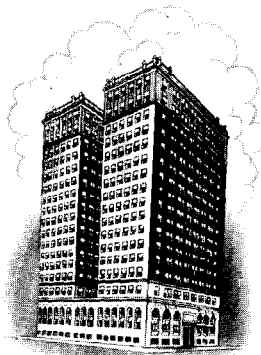


Bradley  
P1  
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**Hotel Sylvania**  
LOCUST STREET JUST OFF BROAD  
PHILADELPHIA 7, PA.

Dear Josh and Esther :

I am a trifle annoyed as I write this letter. I sent Semanti a manuscript, accepted for publication in Genetics. It reports  $S^R \times S^S$  and  $V^S \times V^R$  reciprocal crosses. Almost everything that came out is sensitive. So far so good. Giuseppe kindly sent a manuscript of his. So far so good. He reports on the analysis of "heterogeneous clones" which he calls heteroclones. Note however that he does not purify his recombinants before analyses. I fail to see how one can make quantitative statements (or even claim clones on this basis).

Beyond that, his findings are ~~very~~ precisely those of our group - reported in NY in 1959. He does not cite our work or discuss in any way the unclassifiable and anomalous types described by us. Enough of this petty behavior on my part. I will appreciate your comment though if I am way off base.



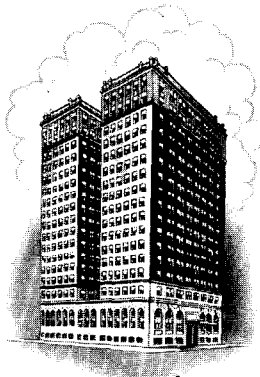
P2

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Perhaps I had better repeat our current views since they have been widely misinterpreted. We believe that ~~the~~ the genetic material of the streptomyces is organized into "chromosomes". The genome tends to behave as a unit; it is not important whether there are more than 1 chromosome so long as it tends to stick together (a la Arell and Suckoria). We do not believe that there is a nuclear membrane. Hyphal fusion therefore "dumps" the two genomes together in the central nuclear vacuole of the cell (which means nothing). There is then nuclear distinction between heterokaryosis and diploidy. Although I have limited views on chromosomal replication, I believe that genetic material between daughter cells is randomly or amitotically distributed. Since we previously postulated that a genome behaves as a unit, every cell or spore

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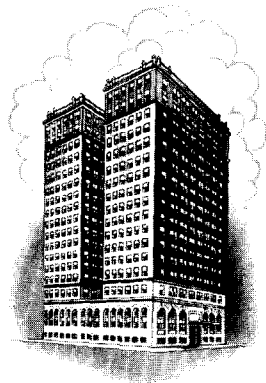
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gets at least one set. We also believe that our strains are polysomic or have fragments and that these may be unequally partitioned out. This concept is principally based on the observation that spores of a single chain though qualitatively alike differ quantitatively (amt of streptomycin to inhibit etc). In view of what Ruth Korman writes about her work, this may have application in true bacteria. I feel it fits the cytological picture better than our current concepts (Kellenberg's photos notwithstanding).

While I have you, I might as well tell you about other events. I visited Davis Cal. recently. Climate was wonderful, space etc not so good. Thanks for keeping me in mind. Actually Jerry Gyertan and I are in pretty good terms right now and we (my lab) are working like the devil and having a wonderful time with everyone leaving us alone. Cf Stanford has any extra students though, we could use

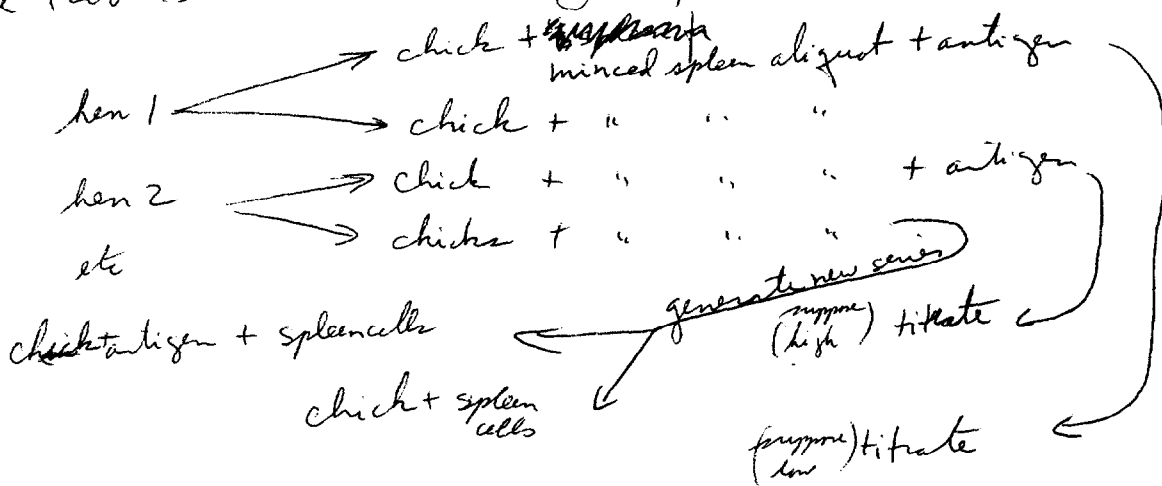


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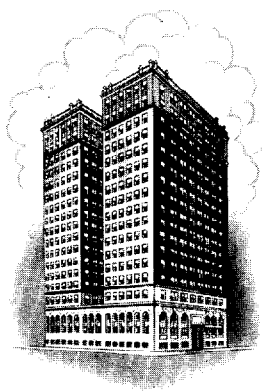
good one. Spizizen here at SAB tells me Nester likes it there with you. He seemed like a nice, bright boy.

The antibody work by Watson and I is underway again. Mutation and selection (I dislike the term clonal theory) look better every day. The two series we have going are:



Another works an indirect selection. The validating of methodology is still going on.

The other is a modified (naturally) Newcombe experiment. It may be based on the faulty hope that at least some of the cells susceptible to mutation to antibody formation and their progeny stay in one place a while.



# Hotel Sylvania

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Well Josh & Esther, I have written a much longer letter than I should. I enjoy my work tremendously. I must confess though, I get terribly lonely for you and for people like you (there aren't many as wonderful as you two).

I sometimes feel I spend too much time encouraging other to an awareness of genetics, and that my own work is too diffuse to be meaningful. I guess this session of the SAB is getting me down.

Lois and the children are fine, they remember you all and the trips to the lake. I extend my very best wishes, deepest affection to you both.

Gaylen.